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THE EVOLUTION OF BOTANICAL RESEARCH¹

A MEETING of the American Association in St. Louis is of special interest to botanists. When this city was little more than a frontier town, Dr. George Englemann became one of its citizens. In spite of his duties as a successful physician, he became one of our greatest botanists. In fact, in the days when taxonomy was practically the whole of botany, and our virgin flora was being explored, the great American trio of botanists was Asa Gray, of Cambridge, John Torrey, of New York, and George Englemann, of St. Louis. Englemann's distinction was that he published no general botanical works, but selected a series of the most difficult problems in taxonomy, and in a masterly way organized for us many perplexing groups. With these groups his name will always be associated. To a botanist, therefore, St. Louis means the home of George Englemann.

There is another association also for the botanist. St. Louis is the home of one of our great botanical gardens, identified for those of us who are older with the name of Henry Shaw; but we are becoming accustomed to its later name, the Missouri Botanical Garden. Its plans and activities represent a fitting continuation of the spirit of Englemann and Shaw, adapted to the progress of botanical science.

In consequence of these associations, St. Louis may be said to have a botanical atmosphere, of which botanists are very conscious. We have the feeling, therefore, not of a visit, but of a home-coming.

A presidential address, delivered to a group composed of investigators representing all the sciences, and including also those interested

¹ Address of the president of the American Association for the Advancement of Science, St. Louis, December, 1919.

in science should deal with some interest common to all. In my judgment our common bond is interest in research; in fact, the major purpose of this association is to stimulate research by the personal contact of investigators. In selecting as my subject, therefore, the evolution of botanical research, I am assuming that the situation developed may apply in a general way to all scientific research.

My purpose is not to outline the history of botanical research, but rather to call attention to certain evolutionary tendencies and to project them into the future. We are all familiar with the gradual historical development of different phases of botany, until botanists became segregated into many distinct groups, the only common bond being the use of plants for investigation. This segregation was for a time very complete, so that the interests of one group would not have been affected if none of the other groups had existed. This monastic phase of botany has subsided somewhat, not for all individuals, but for the subject in general. The different groups are coming into contact and even interlocking, so that the science of botany bids fair to be recognized as an increasing synthesis, rather than an increasing disintegration. In connection with these gradual evolutionary changes, I wish to emphasize three tendencies which seem to me to be significant. As in all evolutionary progress, the tendencies may seem numerous, but the three I have selected seem to me to be especially prophetic of a new era of botanical research.

1. One of the growing tendencies of botanical research is to attack problems that are fundamental in connection with some important practise. The outstanding illustration, of course, is the increasing attention given to the problems that underlie agriculture; but there are many other practises also which are bedded in botanical investigation. We all realize that this tendency was stimulated by the war; in fact, this has been the experience of all the sciences, more notable perhaps in the case of physics and chemistry than in the other sciences, but a very obvious general result. This tendency is so strong at present,

that I do not believe it will ever subside, but it should be understood. There is no evidence that it is tending to diminish research whose sole purpose is to extend the boundaries of knowledge, which all of us must agree is the great objective of research. It merely means that experience developed in connection with an important practise has suggested fundamental problems, whose solution is just as important in extending the boundaries of knowledge as in illuminating some practise. In fact, among our most fundamental problems are those that have been suggested by experience. The injection of such problems among those not related to general experience is not to the detriment of the latter, but simply extends the range of research.

I have no sympathy with the artificial segregation of science into pure and applied science. All science is one. Pure science is often immensely practical; applied science is often very pure science; and between the two there is no dividing line. They are like the end members of a long and intergrading series; very distinct in their isolated and extreme expression, but completely connected. If distinction must be expressed in terms where no sharp distinction exists, it may be expressed by the terms fundamental and superficial. They are terms of comparison and admit of every intergrade. The series may move in either direction, but its end members must always hold the same relative positions. The first stimulus may be our need, and a superficial science meets it, but in so doing it may put us on the trail that leads to the fundamental things of science. On the other hand, the fundamentals may be gripped first, and only later find some superficial expression. The series is often attacked first in some intermediate region, and probably most of the research in pure science may be so placed; that is, it is relatively fundamental, but it is also relatively superficial. The real progress of science is away from the superficial, toward the fundamental, and the more fundamental are the results, the more extensive may be their superficial expression. Not only are practical problems not a detriment to botanical science, but inciden-

tally they strengthen its claim on public interest as a science that must be promoted. As an incidental result, I look with confidence to a future of far greater opportunity for research than has been possible heretofore, research which must be increasingly fundamental and varied. Even if this were not true, my creed for science is that while its first great mission is to extend the boundaries of knowledge, that man may live in an ever-widening horizon, its second mission is to apply this knowledge to the service of man, that his life may be fuller of opportunity. From the standpoint of science, the second may be regarded as incidental to the first, but it is a very important incident, and really stimulates research. In short, I regard this so-called practical tendency in research as being entirely in the interest of research in general, in increasing the range of fundamental problems, in contributing a powerful stimulus, and in securing general recognition of the importance of research.

2. A second tendency, which I regard as more important, is an increasing realization of the fact that botanical problems are synthetic. Until recently a problem would be attacked from a single point of view, with a single technique, and conclusions reached that seemed as rigid as laws from which there is no escape. In plant morphology, for example, and I speak from personal experience, we described structures, with no adequate conception of their functions. Plant physiologists, on the other hand, would describe functions, with no adequate knowledge of the structures involved; while ecologists often described responses, with no adequate knowledge of either structure or function. The same condition obtained in the other segregates of botany. We all recall the time when plant pathologists described and named pathogenic organisms and paid no attention to the disease, which of course is the physiological condition of the plant. In short, not only taxonomists, but all of us, were simply cataloguing facts in a kind of card index, unconsciously waiting for their coordination. This coordination has now begun, and is one of the strong tendencies which is certain to continue. The morphologist is

beginning to think of the significance of the structure he is describing; the physiologist is beginning to examine the structures involved in the functions he is considering; and the ecologist realizes now that responses to environment which he has been cataloguing are to be interpreted only in terms of structure and function. In other words, around each bit of investigation, with its single point of view and single method of attack, there is developing a perspective of other points of view and other methods of attack.

This does not mean a multiple attack on each problem by each investigator. We must remain morphologists, physiologists, and ecologists, each group with its special technique and special kind of data. But it *does* mean a better estimation of the results, a watchful interest in the possibilities of other methods of attack, a general toning down of positiveness in conclusions. We all realize now that plants are synthetic, and that is quite a notable advance from that distant time when we thought of them only as objects subservient to laws of nomenclature. This increasing synthetic view is resulting in a proper estimate of problems. The data secured by each investigation constitute an invitation to further investigation. We have in mind the whole problem and not scraps of information. In short, the synthetic view has developed about our problems the atmosphere in which they actually exist.

3. A third tendency, which seems to me to be the most significant one, is the growing recognition of the fact that structures are not static, that is, inevitable to their last detail. As a morphologist, I may recall to your memory the old method of recording the facts in reference to the development of such a structure as the embryo of seed plants. Not only every cell division in the ontogeny was recorded, but also the planes of every cell division. The conception back of such records was that the program of ontogeny was fixed to its minutest detail. It is probably true that such a structure is about as uniform in its development as any structure can be; but it has become evident now that many of the details recorded were not significant. In-

stead of cataloguing them as of equal value, we must learn to distinguish those that are relatively fixed from those that are variables.

In the same way, much of the older work in anatomy must be regarded as records of details whose relative values were unknown. Even the structures involved in vascular anatomy are not static, but many a phylogenetic connection has been formulated on the conception of the absolute rigidity of such structures in their minutest detail. This conception has made it possible, of course, to develop as many static opinions as there are variables in structure.

Perhaps the greatest mass of details has been accumulated by the cytologists, in connection with their examination of the machinery of nuclear division and nuclear fusion. In no other field has the conception of the rigidity of the structures involved become more fixed, even to the minutest variation in form and position. Of course we all realize that any field of investigation must be opened up by recording all the facts obtained; but we must realize that this is only the preliminary stage. The time has come when even the recorded facts of cytology are being estimated on the basis of relative values; that is, the inevitable things are being differentiated from the variables.

The same situation is developing in the field of genetics. We all recall the original rigidity of the so-called laws of inheritance. It was natural to begin the cultivation of this field with the conception that the program of heredity is immutable, and that definite structures are inevitable, no matter what the conditions may be. There was probably more justification for this conception in this field, on the basis of the early investigations, than in any other, but experience has begun to enlarge the perspective wonderfully. The rapidly accumulating facts are becoming so various that consistent explanations require a high degree of mental agility. More fundamental, however, is the recognition of the fact that the problem of heredity involves not only germinal constitution, which gives such rigidity as there is, but also the numerous factors of environment. In other words, such

problems have become synthetic in the highest degree, making possible results that are anything but static.

In considering these illustrations of the tendency to recognize that facts are not all pigeon-holed and of equal value, it is becoming more and more obvious that our botanical problems are in general the application of physics and chemistry to plants; that *laws*, when we really discover them, are by definition static, but that their operation results in anything but static structures. In other words, structure must respond to law, but the particular law that is gripping the situation may be one of many.

With such evolutionary tendencies in mind, what is the forecast for botanical research? I wish to call attention to three important features that seem certain to characterize it.

1. It will be necessary for the investigator who wishes to have a share in the progress of the science, rather than merely to continue the card catalogue assembling of random data, to have a broader botanical training than has seemed necessary heretofore. Our danger has been that the cultivation of a special technique, which of course is necessary, is apt to limit the horizon to the boundary of that technique. In some cases the result to the investigator has been more serious than limiting his horizon; it has led him to discredit other methods of attack as of little importance. In case this attitude is associated with the training of students, it is continued and multiplied by pedigree culture. The product of certain laboratories is recognized as of this type, and it is out of line with the evident direction of progress.

This demand of the future does not mean that one must specialize less than formerly. It is obvious that with the increasing intricacy of problems, and the inevitable development of technique, we must specialize more than ever. What the new demand means is not to specialize less, but to see to it that every specialty has developed about it a botanical perspective. In other words, instead of an investigator digging himself into a pit, he must do his work on a mountain top. This secures some understanding and appreciation

of other special fields under cultivation, some of which will certainly interlock with his own field. To meet this situation will demand more careful attention to the training of investigators than it has received. Interested and even submerged in our own work, as we must be, still we must realize that the would-be investigator must develop his atmosphere as well as his technique, or he will remain medieval.

To be more concrete, the morphologist in the coming days must appreciate the relation that physiology and ecology hold to his own field. This is far from meaning that he must be trained in physiological and ecological investigation; but he must know its possibilities. The same statement applies in turn to the physiologist and ecologist, and so on through the whole list of specialties.

This first forecast of the future applies to the necessary training of investigators rather than to investigation itself.

2. A second important feature that is sure to be included in the botanical investigation of the future is cooperation in research. During the last few years the desirability of cooperation has been somewhat stressed, and perhaps the claims for it have been urged somewhat unduly. This was natural when we were desiring to secure important practical results as rapidly as possible. It opened up, however, the possibilities of the future. No one questions but that individual research, to contrast it with cooperative research, must continue to break the paths of our progress. Men of ideas and of initiative must continue to express themselves in their own way, or the science would come to resemble field cultivation rather than exploration. It is in this way that all our previous progress has been made. The new feature is that individual research will be increasingly supplemented by cooperative research. There are two situations in which cooperative research will play an important rôle.

The more important situation is the case of a problem whose solution obviously requires two or more kinds of special technique. There are many problems, for example, which a morphologist and a physiologist should at-

tack in cooperation, because neither one of them alone could solve it. Two detached and unrelated papers would not meet the situation. Our literature is burdened with too many such contributions now. The one technique must be a continual check on the other during the progress of the investigation. This is a very simple illustration of what may be called team work. It is simply a practical application of our increasing realization of the fact that problems are often synthetic, and therefore involve a synthetic attack.

Another simple illustration may be suggested. If taxonomists and geneticists should work now and then in cooperation, the result might be either fewer species or more species; but in any event they would be better species. The experience of botanists can suggest many other useful couplings in the interest of better results. In the old days some of you will recall that we had investigations of soil bacteria unchecked by any work in chemistry; and side by side with this were investigations in soil chemistry unchecked by any work with soil bacteria.

Perhaps the most conspicuous illustration of discordant conclusions through lack of cooperation, so extreme that it may be called lack of coordination, may be found in the fascinating and baffling field of phylogeny. To assemble the whole plant kingdom, or at least a part of it, in evolutionary sequence has been the attempt of a considerable number of botanists, and no one of them, as yet, has taken into consideration even all the known facts. There is the paleobotanist who rightly stresses historical succession, with which of course any evolutionary sequence must be consistent, but who can not be sure of his identifications, and still less sure of the essential structures involved. History is desirable, but some real knowledge of the actors who make history is even more desirable.

Then there is the morphologist, who stresses similarity of structures, especially reproductive structures, and leaves out of sight not only accompanying structures but also historical succession.

Latest in the field is the anatomist, especially the vascular anatomist, who compares

the vascular structures in their minutest detail, and loses sight of other important factors in any evolutionary succession.

Apparently no one, as yet, has taken all the results from all fields of investigation, and given us the result of the combination. In other words, in phylogeny, we have had single track minds. This has been necessary for the accumulation of facts, but unfortunate in reaching conclusions.

This is but a picture of botanical investigations in general as formerly conducted; and it seems obvious that cooperative research will become increasingly common as cooperation is found to be of advantage.

The second situation in which cooperative research will play an important rôle is less important than the first, but none the less real.

It must be obvious to most of us that our literature is crowded with the records of incompetent investigations. Not all who develop a technique are able to be independent investigators. They belong to the card catalogue class. They are not even able to select a suitable problem. We are too familiar with the dreary rehearsal of facts that have been told many times, the only new thing, perhaps, being the material used; and even then the result might have been foretold. It is unfortunate to waste technique and energy in this way; and the only way to utilize them is through cooperative research, for which there has been a competent initiative, and in the prosecution of which there has been a suitable assignment of parts. In my judgment this is the only way in which we can conserve the technique we are developing, and make it count for something. I grant that the product of such research is much like the product of a factory, but we may need the product.

In one way or another, cooperative research will supplement individual research. Individuals, as a rule, will be the pioneers; but all can not be pioneers. After exploration there comes cultivation, and much cultivation will be accomplished by cooperation.

3. The most important feature that will be developed in the botanical investigation of the future is experimental control. Having rec-

ognized that structures are not static, that programs of development are not fixed, that responses are innumerable, we are no longer satisfied with the statement that all sorts of variations in results occur. We must know just what condition produces a given result. This question as to causes of variable results first took the form of deduction. We tried to reason the thing out.

A conspicuous illustration of this situation may be obtained from the history of ecology. Concerned with the relation of plants to their environment, deductions became almost as numerous as investigators. Even when experimental work was begun, the results were still vague because of environment. Finally, it became evident that all the factors of environment must be subjected to rigid experimental control before definite conclusions could be reached.

What is true of ecology is true also of every phase of botanical research. For example, I happen to be concerned with materials that showed an occasional monocotyledonous embryo with two cotyledons, while most of the embryos were normal. The fact of course was important, for it connected up Monocotyledons and Dicotyledons in a very suggestive way, and also opened up the whole question of cotyledony. Important as the fact was, much more important was the cause of the fact. We could only infer that certain conditions might have resulted in a dicotyledonous embryo in a monocotyledon; but it was a very unsubstantial inference. That problem will never be solved until we learn to control the conditions and produce dicotyledonous embryos from Monocotyledons at will, or the reverse. Comparison and inference must be replaced by experimental control; just as in the history of organic evolution, the method shifted from comparison and inference to experimental control. It will be a slow evolution, and most of our conclusions will continue to be inferences, but these inferences will eventually be the basis of experiment. In fact, most of our conclusions are as yet marking time until a new technique enables us to move forward.

These illustrations from ecology and morph-

ology represent simple situations as compared with the demands of cytology or genetics, but the same need of experimental control is a pressing one in those fields. The behavior of the complex mechanism of the cell is a matter of sight, followed by inference, when we know that invisible factors enter into the performance. How the cell program can ever be brought under experimental control remains to be seen, but we must realize that in the meantime we are seeing actors without understanding their action. In fact, we are not sure that we see the actors; the visible things may be simply a result of their action. The important thing is to keep in mind the necessary limitations of our knowledge, and not mistake inference for demonstration.

Even more baffling is the problem of adequate experimental control in genetics. We define genetics as breeding under rigid control, the inference being that by our methods we know just what is happening. The control is rigid enough in mating individuals, but the numerous events between the mating and the appearance of the progeny are as yet beyond the reach of control. We start a machine and leave it to its own guidance. The results of this performance, spoken of as under control, are so various, that many kinds of hypothetical factors are introduced as tentative explanations. There is no question but that this is the best that can be done at present; but it ought to be realized that as yet no real experimental control of the performance has been devised. The initial control, followed by inferences, has developed a wonderful perspective, but a method of continuous control is yet to come.

Having considered the conspicuous evolutionary tendencies of botanical research and their projection into the future, it remains to consider the possible means of stimulating progress. It will not be accomplished by increasing publication. It is probably our unanimous judgment that there is too much publication at the present time. What we need is not an increasing number of papers, but a larger percentage of significant papers. This goes back to the selection of problems, assuming that training is sufficient. A leader

is expected to select his own problems, but we are training an increasing army of investigators, and the percentage of leaders is growing noticeably less. There ought to be some method by which botanists shall agree upon the significant problems at any given time, in the various fields of activity, so that such advice might be available. It is certainly needed.

I realize that our impulse has been to treat a desirable problem as private property, upon which no trespassing is allowed. Of course, common courtesy allows an investigator to work without competition; but the desirable problems are still more numerous than the investigators; and we must use all of our investigative training and energy in doing the most desirable things. There need be no fear of exhausting problems, for every good problem solved is usually the progenitor of a brood of problems. We will never multiply investigators as fast as our investigations multiply problems. In the interest of science, therefore, we should pool our judgment, and indicate to those who need it the hopeful directions of progress.

Not only is there dissipation of time and energy in the random selection of problems, but there is also wastage in investigative ability. Every competent investigator should have the opportunity to investigate. The pressure of duties that too often submerge those trained to investigate is a tremendous brake upon our progress. I am not prepared to suggest a method of meeting this situation, but the scientific fraternity, in some way, should press the point that one who is able to investigate should have both time and opportunity. A university regulation, with which we are all too familiar, which requires approximately the same hours of all of its staff, whether they are investigators or not, should be regarded as medieval.

In conclusion, speaking not merely for botanical research, but for all scientific research, it has now advanced to a stage which promises unusually rapid development. The experience of the recent years has brought science into the foreground as a great national asset. It should be one of the func-

tions of this great association to see to it that full advantage is taken of the opportunity offered by the present evolutionary stage of research and public esteem. We must choose between inertia and some display of aggressive energy.

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TIME, SPACE, AND GRAVITATION¹

AFTER the lamentable breach in the former international relations existing among men of science, it is with joy and gratefulness that I accept this opportunity of communication with English astronomers and physicists. It was in accordance with the high and proud tradition of English science that English scientific men should have given their time and labor, and that English institutions should have provided the material means, to test a theory that had been completed and published in the country of their enemies in the midst of war. Although investigation of the influence of the solar gravitational field on rays of light is a purely objective matter, I am none the less very glad to express my personal thanks to my English colleagues in this branch of science; for without their aid I should not have obtained proof of the most vital deduction from my theory.

There are several kinds of theory in physics. Most of them are constructive. These attempt to build a picture of complex phenomena out of some relatively simple proposition. The kinetic theory of gases, for instance, attempts to refer to molecular movement the mechanical thermal, and diffusional properties of gases. When we say that we understand a group of natural phenomena, we mean that we have found a constructive theory which embraces them.

THEORIES OF PRINCIPLE

But in addition to this most weighty group of theories, there is another group consisting of what I call theories of principle. These employ the analytic, not the synthetic method. Their starting-point and foundation are not

hypothetical constituents, but empirically observed general properties of phenomena, principles from which mathematical formulæ are deduced of such a kind that they apply to every case which presents itself. Thermodynamics, for instance, starting from the fact that perpetual motion never occurs in ordinary experience, attempts to deduce from this, by analytic processes, a theory which will apply in every case. The merit of constructive theories is their comprehensiveness, adaptability, and clarity, that of the theories of principle, their logical perfection, and the security of their foundation.

The theory of relativity is a theory of principle. To understand it, the principles on which it rests must be grasped. But before stating these it is necessary to point out that the theory of relativity is like a house with two separate stories, the special relativity theory and the general theory of relativity.

Since the time of the ancient Greeks it has been well known that in describing the motion of a body we must refer to another body. The motion of a railway train is described with reference to the ground, of a planet with reference to the total assemblage of visible fixed stars. In physics the bodies to which motions are spatially referred are termed systems of coordinates. The laws of mechanics of Galileo and Newton can be formulated only by using a system of coordinates.

The state of motion of a system of coordinates can not be chosen arbitrarily if the laws of mechanics are to hold good (it must be free from twisting and from acceleration). The system of coordinates employed in mechanics is called an inertia-system. The state of motion of an inertia-system, so far as mechanics are concerned, is not restricted by nature to one condition. The condition in the following proposition suffices: a system of coordinates moving in the same direction and at the same rate as a system of inertia is itself a system of inertia. The special relativity theory is therefore the application of the following proposition to any natural process: "Every law of nature which holds good with respect to a coordinate system K must also hold good for any other system K' provided

¹ From the *London Times*.